a glass vessel? In his letter of February 14, Prof. Dewar tells us that by means of the copper coil apparatus of 1886 "liquid oxygen was made and decanted or transferred from the vessel in which it was liquefied to another by means of a valve, and thereby rendered capable of use as a cooling agent." Why, then, did he not use the liquid oxygen as a cooling agent? It is strange that seven years after this, when Prof. Dewar wished to "prosecute research at temperatures approaching the zero of absolute temperature," he says he had "no recorded experience to guide [him] in conducting such investigations." Had he forgotten his own apparatus with its copper coil 45 feet long?

Prof. Dewar's apparatus of 1886 must have been peculiar; in one sentence in his letter of February 14, the deviser speaks of it as "entirely different in type from the crude plan Olszewski adopted in 1890," and in another sentence he assures us it was "practically identical in principle with that used in Cracow in 1890."

The apparatus of 1886 succeeded, in Prof. Dewar's hands, in producing solid oxygen; why have we heard nothing of that solid oxygen? Determinations of some of the constants of that substance would be interesting and important.

I must beg attention to an instance of Prof. Dewar's boldness. In 1883 Messrs. Wroblewski and Olszewski liquefied oxygen, and other gases, in a strong glass tube; in 1884 Prof. Dewar repeated some of the experiments of the Polish Professors, using, as they had done, a strong glass tube, but adapting some parts of their apparatus so as to make it "for the purpose of lecture demonstration . . . more readily and quickly handled"; in 1890 Prof. Olszewski described an improved and enlarged instrument, based on that described by his colleague and himself in 1883, wherein the liquefaction was effected in a steel cylinder. Yet, in his letter to NATURE of February 14, Prof. Dewar says "replace the glass tube in *my* apparatus of 1884 by a small steel cylinder, and attach to its lower end a narrow copper tube with a stopcock, and the Olszewski apparatus of 1890 is produced." (The italics are mine.)

(4) As regards Prof. Dewar's joint claim to priority in the determination of the refractive index of liquid oxygen, I admit he is right and I was wrong. I was misled by the date at the beginning of the paper which contains the earliest measurements by Messrs. Olszewski and Witkowski of the refractive index of liquid oxygen. The paper is headed *Extrait du Bulletin de l'Académie des Sciences de Cracovie, Octobre* 1891. At the end of the paper is the date 15 *Juillet* 1892; this, and the footnote, quoted by Prof. Dewar, make it evident that 1891 at the beginning of the paper is a printer's error for 1892. I am sorry I made this mistake. Nevertheless, considering that Olszewski's quantitative experiments on the optical properties of liquid oxygen were begun in 1887 (*Wied. Ann.* xxxiii., 570), and were continued in January 1891 (*ibid.* xlii., 633), I hold I was justified in referring to Prof. Dewar's experiments on the optical properties of liquid oxygen as "mainly repetitions of the work of Olszewski and Witkowski."

Towards the end of his letter Prof. Dewar refers to a quotation I made from one of his lectures ("Having no recorded experience to guide us . . ."); he follows my quotation (of which he gives only a part) by another to the effect that "the necessity of devising some new kind of vessel for storing and manipulating exceedingly volatile fluids, like liquid oxygen and liquid air, became apparent when the optical properties of the bodies came under examination." I had expressed admiration "of the skill which produced" Prof. Dewar's new vacuum receivers. Prof. Dewar now tells us that when he prefaced his account of the making of these receivers by the sweeping assertion that he had no experience to guide him in conducting investigations at very low temperatures, he did not mean what he said; it was merely his way of saying that the vacuum receivers he was about to describe had not been made before. It was not I who made, it was Prof. Dewar himself who has made, a "glaring misrepresentation of the meaning" of his own words.

Prof. Dewar lays no stress, in his letter, on his own experiments on chemical action, phosphorescence, &c., at very low temperatures. It is these experiments, I suppose, that are referred to when he speaks of "throwing his bread upon the waters"; or does he abandon them as trifling, in that strange sentence—"Have I ever suggested that Prof. Olszewski was anticipated, or attempted to raise any question of priority?"?

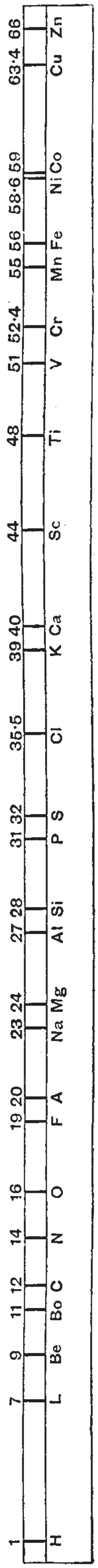
Prof. Dewar hides the essential questions in a mist of words. If he has made marked improvements in the methods of liquefying and manipulating the more permanent gases (besides his invention of vacuum receivers); if he has conducted original, accurate, and thorough investigations into the properties of the liquefied gases, where are the accounts of this work to be found? Every student of the subject knows he can lay his finger on the work of Olszewski, and also on that of his deceased colleague Wroblewski; and he knows that work to be thorough, accurate, and important. But where can there be found some distinct account of the scientific work in connection with the liquefaction of the more permanent gases that is attributed to the Fullerian Professor at the Royal Institution?

I asked, and I still ask, for an "instant and serious consideration" of the whole matter brought forward in these letters by "those who are truly interested in the advance of science, and are jealous of the good name of the scientific men of this country."

M. M. Pattison Muir.

Cambridge, February 17.

Argon.



The beautiful research of Lord Rayleigh and Prof. Ramsay has proved up to the hilt that in argon they have discovered an unknown gas which is remarkable for some of its physical properties, and especially for its extraordinary chemical inertness.

Among the interesting questions which have been raised, but not fully solved, are: Is argon an elementary body? If so, how does it stand related to the other elements?

There are some who advocate its being an allotropic form of a previously known element, just as ozone is allotropic oxygen; but it has not yet been produced from, or resolved into, nitrogen or any other element; and, until that should happen, it has the full right to take rank as a new element.

At first sight the fact of its giving two different line spectra under different circumstances might favour the idea of its being resolved into two bodies; but the fact that these two very compound line spectra have twenty-six lines in common, which are not all of them among the strongest lines, appears to me an argument for the fundamental unity of the substance.

If argon be an element, what is its place in Mendeléeff's table? This is at present agitating the minds of chemists and physicists. The specific gravity of the gas would lead to an atomic weight of close upon 20; but it is argued from the velocity of sound through it that it is a monatomic gas, and therefore close upon 40. Now, if 20 be the atomic weight, it will fall in admirably with the periodic law; if 40, it will be perfectly inconsistent with it. The argument in favour of 20 is at least five-fold:—

(1) It will fill an existing vacancy in Mendeléeff's arrangement; that at the top of the eighth column. This is the first "even" series; and it should be remembered that the later "even" series in that last column are represented by the iron, palladium and platinum groups of metals; the "uneven" series being as yet unrepresented.

(2) It will follow the periodic law in regard to its melting point. That should be very low, like nitrogen, oxygen and fluorine; and so it is.

(3) It will follow the law in regard to its atomic volume. That is small; and so it should be by analogy.

(4) A great characteristic of argon is its not forming stable compounds with other bodies, at any rate at a temperature high above its boiling

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point. This property characterises the other occupants of the eighth column; to some extent in the iron group, and certainly in the palladium and platinum groups.

(5) The introduction of a new element with the atomic weight of 20 (not 21 or 22), will extend the range of certain recurring numbers which appear near the beginning of the series of atomic weights. In the existing arrangement of the atomic weights it will be observed that between oxygen 16 and fluorine 19 there are three units; between fluorine 19 and sodium 23 there are four; then the numbers run 1, 3, 1, 3, 1, 3½. Adding argon at 20 the series becomes symmetrical all the way from oxygen to chlorine, as will be seen in the diagram on the previous page.

If, on the other hand, we were to adopt 40 as the atomic weight of argon, we should meet with the following serious difficulties:—

(1) There is no room for it. To place another figure just before or just after calcium would disarrange the whole subsequent series.

(2) It would break the periodic law in regard to its melting point.

(3) It would break the law in regard to its atomic volume.

(4) The inactive argon would be associated with metals of the earths, the compounds of which are remarkably stable.

(5) It would bring the atomic weight of three elements, potassium 39, calcium and argon each about 39·9, within one unit. This never occurs elsewhere in Mendeléeff's table.

Against these considerations there is the forcible argument deduced from the ratio of the specific heats of argon. I will not attempt to weigh the respective merits of these lines of reasoning, especially in the absence of the details of the experiments on the velocity of sound, and until we have some knowledge of the compounds of argon. Trustworthy conclusions will not be possible till this further information is obtained. It is not a question of physics *versus* chemistry, for the true theory of its place among the elements must be able to coordinate all the facts upon which both the chemist and the physicist rely.

J. H. GLADSTONE.

London, February 8.

The Aurora of November 23, 1894.

PERMIT me to call attention to a significant fact disclosed by a scrutiny of the observations of the aurora of November 23, 1894, and having an important bearing in discussing the auroral dimensions, and which appears to have escaped notice (see NATURE, January 10). I refer to the *invisibility* of the objects at Dingwall, to the observers at Tynron, Dumfriesshire. Extracts from the synchronous accounts in NATURE, November 29, p. 107, and January 10, p. 246, will prove this statement.

Tynron, 7.30.—Luminous mist in the northern sky, strong enough to cast shadows on the shining surface of the wet ground. The mist moved from the horizon to the zenith, forming a detached luminous belt in patches, disappearing at 8.30, leaving only the light in the north.

Dingwall, 7.30.—Sky covered in all directions by a canopy of streamers. At the same time the arch disappeared, and occasional streamers up to eight o'clock. It is not possible that the arch seen at Dingwall could be the same as the one at Tynron, because the former had vanished when the latter commenced to form, whilst there is a total absence of streamers, and phenomenally brilliant mist not recognised at the other place. Until methods of observation and analysis can be introduced that will eliminate the errors of identification, the solution is likely to be indefinitely postponed.

W. H. WOOD.

Birmingham, February 3.

MAKING the necessary allowances for increased apparent luminosities of bright streaks, or of layers of light in the atmosphere, by the foreshortening effects of end-on, and of edge-presentations, the observations at Dingwall and in Dumfriesshire of the aurora of November 23 last, scarcely seem to recount very much which was not at the two places, at least partially, a fairly comparable and nearly contemporaneous description of the same phenomena. The first-formed light-band of the glow was very strong at Dingwall from the east to west, a little southward from the zenith, until 7.30 p.m., when with the usual drift of such displays to southwards, it became less prominent there than the approaching canopy of streamers which supervened, drifting up from the north-eastern sky in rear of it. But it assumed at the same time increasing prominency in Dumfriesshire (150 miles south of Dingwall), where between 7.30 p.m. and 8 p.m., it passed overhead in the shape of detached patches of light, a form which the belt was also seen to assume and to break up into, in a slow extinction stage at Slough, in that interval. The display of streamers rising from a large tract of light-mist approaching Dingwall from the northward and increasing constantly in lustre at 7.30 p.m., did not extend far south of Dingwall before it faded out soon afterwards; and being (as it was seen at Slough) a dense local discharge of them, its corona of brightly-foreshortened beams overhead would naturally be a very impressive sight at Dingwall, although from a position 150 miles distant in Dumfriesshire, the broadside aspect of the short outbreak, seen from afar, instead of from underneath, would only have the appearance of a sheaf of coloured light projected up from the usual flat streamer-base, neither very wide nor extraordinarily lofty, but of the massive berg-like form, which was its description at Slough, not unfrequently noticeable in rather strong auroras; and it may even have been quite easily hidden from view entirely at Tynron, although a clear horizon in the north near Slough allowed its observation there, by trees or by other obstructions in the landscape.

If Mr. Wood can happily devise a practical means of perfectly recording the times and descriptions of *all* the rapidly changing features of an aurora, and the shifting variations of its misty light-glows, he could no doubt achieve results from observations which would be no less a benefit to astronomers and terrestrial magneticians, than an exact continuous registration of cloud phenomena would be of welcome interest to meteorologists and terrestrial electricians; but it needs no great familiarity with auroral exhibitions to be quite certainly assured that results of records even so elaborate as those might be, would never be found to confute, but only to confirm the generally accepted view of the really cosmical heights and dimensions of all truly auroral lights and corruscations; of all such lights, that is to say, as show in their spectra the yellowish auroral line, or some of the other well-recognised spectral indices of the aurora.

A. S. HERSCHEL.

The American Association.

AT the Brooklyn meeting of the American Association for the Advancement of Science last summer, it was decided to meet this year at San Francisco, provided reasonable rates of fare could be secured from the trans-continental routes, as it was supposed could be done. Prof. Joseph Le Conte for three consecutive years had crossed the continent, laden with earnest and cordial invitations from the Universities and Scientific Societies of California, and the Common Council of San Francisco, to hold the meeting for 1895 in that city. The short-sighted policy of the railroads, however, refused to grant any concessions; and it has at last been decided to meet at Springfield, Mass., August 29—September 4. The meetings of affiliated societies will begin on Monday, August 26—rather later than the usual time of meeting.

Springfield is a small city, located in the heart of New England. It is the seat of the principal arsenal of the United States; and, while not a University city itself, it is within two or three hours' ride of nearly, or quite, a score of institutions of learning of the highest grade, including the two oldest and most powerful Universities in America, Yale and Harvard. This will be the second meeting at Springfield, the first having been held in 1859.

The Association is incorporated by the State of Massachusetts, and its office and museum are at Salem in that State; but no meeting has been held in New England since that at Boston in 1880, the most brilliant in all the history of the Association. The return to New England after this longest absence, gives unusual interest to the approaching meeting.

WM. H. HALE.

Brooklyn, New York, February 2.

Earthquake in Norway.

ABOUT midnight on the 4th and 5th of this month, a fairly strong earthquake occurred in the southern part of Norway. The greatest disturbance was felt in the environs of the town of Aalesund, upon the west coast (about 60° 30′ lat. N.). From there

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